ON THE INHERITANCE OF RHYTHM

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A. INTRODUCTION

It is perhaps unfortunate that the term rhythm must be used in this investigation, but usage seems to demand it. The objection to the term is that it may have three distinct meanings, physical, physiological, and psychological. The last of these concerns us here. There is certainly no danger of confusing the psychological with the physical rhythms; but confusion of the two organic 'rhythms,'—the physiological and the psychological,—is of common occurrence. For this reason it would seem well to point out the difference between the two by the following concrete example. The physiologist says that normal walking is a rhythmical act. Whenever a man is so badly paralyzed in one of his limbs that he is obliged to give the diseased member a peculiar swing in walking, the physiologist may use normal walking as a control and say that the afflicted man walks non-rhythmically. On the other hand, the psychologist ought to call the 'hobbling metre' movements of the paralyzed person a two-rhythm. The chief characteristic of *rhythm to the psychologist is the systematic accentuation and subordination of the elements of a series.* This interest at once eliminates all physiological and physical rhythms from the field of psychology.

Eugene Landry[1] has very well discussed the different conceptions of rhythm. His discussion was necessitated by his very broad definition of the term rhythm as 'la marche de l'énergie.' This would mean any activity is rhythmical where there is a succession of varying intensities. So he speaks even of the rhythm of prose recitation. He is not interested in all the intensity successions possible anywhere in the world, but primarily in the change of the energy of human action, and especially speech, from moment to moment,—in what is called psychological rhythm.

Views of rhythm, dating from early Greek mythology to the present, are based on the assumption that rhythm is instinctive. In earlier writings the hypothesis is explicitly stated, but in later editions there was apparently no use in stating a

[1] A bibliography is appended at the end of this paper.

theory so well known. The old assumption is never contradicted; and it is only by assuming the innateness of rhythm that one can justify the problems investigated and the attitude in general of the experimenters. There is only one other hypothesis that can be substituted for the old one, namely, that rhythm in general is not instinctive, or in other words, that it is the result of habit. If, with this assumption in mind, one undertakes a review of the literature on rhythm, one is bound to be shocked by the preponderance of pseudo-problems and by the great amount of attention that has been given to them. Perhaps it would not be out of place to examine a few of the most recent investigations in order that their contradictions may be shown, and also that the status of affairs concerning the problem of the origin of rhythm may be indicated.

Warner Brown undertook 'to determine which is the more essential to rhythmical grouping, the uniform time of recurrence or the uniform character of the thing that recurs.' The conclusion drawn by this investigator leaves the impression that if the phenomenon of rhythm is to be understood, it must be attacked from the temporal point of view. Lotze, Herbart, Wundt, and Titchener are supporters of this view. Brown speaks of *temporal rhythm* and of *accentual rhythm*. As a result of his work he is led to conclude that 'time aspects are fundamental and the accentual features, while necessary, are not at the root of the phenomena' because his experimental data show that 'on the whole, the temporal structure was maintained twice as well as the accentual.'

Another investigation is that of J. E. Wallace Wallin who was concerned with the extent to which the time of rhythmical groups may be varied without appreciably affecting the rhythm. Concerning this problem he concludes that: 'Absolutely periodic or regular occurrences are not essential to the appreciation of rhythm. To engender a feeling of rhythm always requires a certain amount of periodicity; but the margin of irregularity which may obtain is quite considerable.'

It seems rather remarkable that his conclusions should so explicitly contradict those of the first-mentioned investigator. It is in view of the following evidence that he drew the above conclusion. He was able to distinguish five grades or qualities of rhythm in relation to the amount of irregularity introduced between the beats of the rhythmical series. He speaks of five grades of rhythm,—excellent, good, medium, poor, and disrupted rhythms. The average amount of time-displacement or irregularity, in terms of percentage from the average, amounts

respectively to 6.36 (grade excellent), 8.53, 12.0, 14.5, and 17.8 (grade disrupted). In absolute units of time these figures amount to 0.0526, 0.0734, 0.0991, 0.1182, and 0.1488 sec.

Herbert Woodrow in a paper entitled 'A Quantitative Study of Rhythm' dealt with a two-rhythm and found that he could change it from an iambic (accent on the first syllable) to a trochaic (accent on the second syllable), and *vice versa* by lengthening either the time-interval following or that preceding the accent. By a like process with the three-rhythm he was able to pass from dactylic (accent on the first syllable) to anapaestic (accent on the third syllable), and *vice versa.* Other important facts found by Woodrow are the following:

(*1.*) The amount of rhythm as determined by the indifference-point may vary with the intensity of the stimulus. That particular point between two forms at which the nature of the second perceived rhythm is unpredictable, the author terms the indifference-point. This is the measure of the amount of rhythm. (*2.*) The amount of rhythm is found to increase with the rate.

Woodrow believes that rhythmical grouping is altogether a matter of temporal relations; and the effect of the accent in determining grouping is to produce temporal illusions through the over-estimation of the interval preceding the accent.

In another experiment, Woodrow discusses the 'Rôle of Pitch in Rhythm.' Here he employed the same method of measuring as before, and determined that giving one of the beats a pitch differing from that of the others does not influence the accent. Here, too, part of the paper is taken up with the consideration of the rate of recurrence of the rhythm elements. From the introspections of his observers he judged that the preferred rates of auditory stimulation range from 0.305 to 1.37 sec.

Much work has been done on the preferred tempo and there seem to be about as many different conclusions as there are investigators. For example, this tempo, according to Vierordt, is 0.62 sec, according to Stevens its lower limit is 0.53 and the upper limit is 0.87 sec, for Martius it is 0.50 sec; for Meumann it is 0.40 sec. These statistics, in the light of the theory of rhythm to which this present investigation is devoted, mean only that different people seldom make like movements with like speeds. The particular tempo which a person may choose depends in large measure upon the rapidity with which he ordinarily makes movements. This can be verified as I have done by correlating a person's reaction-time and his tempo. A high correlation is always present.

Another investigator who is generally considered to have contributed much to the understanding of rhythm is Bolton. He stated that it was his aim to reduce rhythm to a more fundamental activity of the mind. In the first part of his paper the author discusses all kinds of periodic movements from 'cosmic rhythms' to 'the incubation of fowls;' and in the meantime he shows how 'organic rhythms' have been decidedly affected by 'cosmic rhythms.' After this he talks about 'physiological rhythms,' which are no more than periodic recurrences of certain physiological functions. 'Of these, walking and speech are the most important and are true types of rhythmical activity.' All such movements he calls rhythms. These, however, are not the movements which he investigated; in his experiments he was interested chiefly in what he termed secondary rhythms. A secondary rhythm is derived from a rhythmical series of elements, according to Bolton, by accentuating one of the elements through increasing its intensity, pitch, or tone-color, etc., at regular intervals. 'Accent simply arranges the materials already rhythmical through some temporal recurrence.'

In speaking of rhythms in poetry, Bolton states that the number of accents to the verse may be four, six, or eight. The eight, however, fails to become popular because it exceeds the mental span. Because of the limitation of the mental span, the accents are limited to those two or three numerically small quantities; and it is only for the sake of variety that verses are made to contain five and three accents. Two, although it is the most primary rhythm, according to his way of expressing, seems never to have been used thus because of its extreme simplicity.

In ordinary life, groups of two things and multiples of two occur more often than three, hence rhythmic groups of two and four occur more often than of three. In a conclusion, however, the author makes the statement that 'a member of a sequence may contain *one* or *more* simple impressions,' which expression does not exclude any number.

The author raises the question of the inherent nature of a rhythmic group. The following quotation may serve to show his attitude toward this question.

'The conscious state, accompanying each wave of attention, groups together or unifies all the impressions that fall within the temporal period of the wave. As a result of a number of attentive efforts, a series of auditory impressions takes the form of a sequence of groups. This rhythmical grouping is due to the unifying activity of the mind. . . . . Each suc-

ceeding wave groups a like number of elements, so that the series is conceived in the form of groups. The rhythmical grouping is an attempt to conceive a series of sounds in a simpler form.'

It seems to be by means of this unifying activity of the mind that a series is transformed into a 'secondary rhythm.' The author's last statement, taken in connection with what he has said about the most popular rhythm, would mean that the four-groups and the six-groups are simpler forms than five-groups. If, however, all is dependent on the wave of attention, then groups of four and of six should be no more prevalent than five-groups. According to Bolton's theory of attention waves, it would seem that the five-rhythm must occur more often than the six-rhythm, since it is temporally shorter. From this dilemma his theory never escapes.

Stetson may be considered as beginning where Bolton left off. He is interested to know why the 'mind' possesses this unifying activity, or, as he would probably say, "why the human organism tends to divide a series of elements into unit groups." He is the first to see that the problem of rhythm is a problem of the activity of an organism rather than a problem of temporal consciousness. He is concerned with the causes of the organism's preference for certain groups of muscular activities,—why certain groups of particular numerical values are performed more often than others. Stetson was the first to mourn the fact that rhythm has always fallen into the hands of the investigators of attention, or the span of consciousness, or the perception of time. He says that not the temporal relations but the movements involved are the fundamental things to study if we intend to understand the phenomenon. He states further that it is a sheer assumption that regularity is characteristic of the pure rhythm, since it is easily proved that very wide irregularities can be introduced into a simple sound series without destroying the rhythm. He speaks of the human body as a device for producing rhythm. The larger muscles of the arm, for example, perform heavy movements, while the fingers execute finer movements. Repetition of this movement of the organ (the arm) naturally means a series of accented and unaccented elements of movement at *comparatively* regular intervals. "The unit group is the form in which the various muscle-sets and segments of a limb or organ can all work together, freely and easily, in a single movement cycle." The muscles of the arm can be spoken of as the major and minor muscles, the former performing the accentual movements, the latter making the finer movements.

Furthermore, combining several organs we find, for example, that the hand may make a series of movements which may be accentuated by a foot movement, or, perhaps, by a movement of the entire body. These movements may require a short time or they may require a long time. The speed differs with the particular nature of the parts of the body which move.

Stetson makes clear why two and three, including their powers and products, should be rhythms; but he does not explain any real distinction between five and three. The following quotation is his only attempt to explain why we often find unified movements (5 and 7 perhaps) in our experience. "The unity of an act seems to depend on the continuous character of its constituent movements and on the purposive habit which gave rise to it, rather than on the anatomical relation of the parts involved." But, inconsistently, he considers that five and seven are not rhythms. This may enable us to understand why he says that accentuation and subordination are '*perhaps*' the essential elements in rhythmic perception. He seems to believe that there is possibly another essential element hitherto unknown. If these two, accentuation and subordination, are the all-important factors, then why is it not possible to call five a rhythm quite as well as four? If four is a rhythm and five is not, then accentuation and subordination may not be of chief importance, because elements can be accentuated and subordinated to form groups of five just as surely as to form groups of four.

I am left to infer from Stetson's paper that he regards rhythm as instinctive. The essential element that is above referred to as hitherto unknown, seems to be regarded by Stetson as being of the nature of an instinct. He does not contradict the old view that rhythm is an instinctive something. Furthermore this is the only assumption that makes clear his attitude, viz.: "There is no reason for assuming that the nature of the unit-group of verse differs from that of other rhythms." "Most recent writers are inclined to reduce the types of feet to four; iambic, trochaic, dactylic, and anapaestic. All of the numerous kinds of feet occasionally given can be separated into these elementary forms." "The ordering of the unit-groups into larger unities is possibly a matter of historical development and might be studied in primitive art works."

With Robert MacDougall time is not such an important factor as it is with many other investigators. He concerned himself with many different problems, some of which I shall not discuss in this paper. I wish to speak of his work chiefly since he, like Stetson, superimposed his problems upon a hy-

pothesis of instinctive numerical selection of group elements. This hypothesis is made no more explicit than Stetson makes it; but I think I am justified in simply saying that he assumes, as does Stetson, that rhythm is instinctive. Both are concerned with rhythm as a particular form of activity peculiar to certain animals and not to others. In other words, they are concerned with the fact that human beings group their actions into certain unit groups; that human beings, therefore, possess this instinct.

MacDougall places emphasis on the fact that the human organism prefers certain numerical groups of movements to others. The preferred groups are spoken of as rhythmical. The other groups do not concern the investigator after they are once determined to be non-rhythmical. He makes no search for an organic law to explain why one group should be preferable to another. To overcome the difficulty he assumes, at the start, that rhythmical action is innate, thus assuming at once the right to exclude from the investigation all group-movements which are not innate, as being non-rhythmical. His non-rhythmical groups are the prime numbers higher than three. He does not explain the selective principle which causes the human organism to choose either a four or a six in preference to a five-rhythm. This phenomenon can be explained, as we shall see, only in terms of organic activity as dependent upon the arrangement of parts of our mechanism. MacDougall convinced himself that the seven movement is not innate (and is consequently not a rhythm), and substantiated his conviction by introspections from his subjects, who said that seven furnished no feeling of rhythm. He calls the movements of two and three simple and fundamental rhythms; the movements of four and six, complex and secondary; and the movements of five, seven, or eleven, no rhythms at all. One of these introspective statements is as follows: "The sense of equivalence fell off at five and practically disappeared at seven beats, while groups of six and eight retained a fairly definite value as units in a rhythmical sequence." He obviously means by this statement that the ability to accent every second or every third element is instinctive and that four, six or eight furnish experiences of rhythm because they have the instinctive rhythms as their bases. In performing the nine-rhythm the three is still performed; but each unitary group of three becomes one element for the secondary rhythm of three or nine. The nine is a rhythm superimposed on the three having as its elements the structural units of the three.

I have not at all attempted an exhaustive review of the literature on rhythm. I have only mentioned in a brief way

the contributions of some more recent investigators, as examples of those (the majority) who were chiefly interested in the time aspects of rhythmical action and of those (the minority) who subordinated this problem to others.

The contradictions noted in some of the preceding studies may not only cause one to be doubtful as to the usefulness of the primitive assumption (of instinct), but may be the cause for his becoming an aggressive skeptic. Let us now examine the following experiment which is designed to test the usefulness of our new assumption of rhythm as a habit.

B. Experimental

a. First Experiment

In this investigation I wish to concern myself with rhythm as a problem of organic activity of animal or human behavior. For some reason a characteristic of human beings is to divide repeated movements into certain numerical groups, or rhythmical units. A normal adult, if asked to beat a long succession of like strokes or to listen to such a succession of sounds manifests a tendency to group the elements into periods, that is, into successive unit groups numerically the same. The grouping factor may be an exceptionally extended pause, an exceptionally heavy stroke, a change in pitch, or any other means, although we need not assert that any or all would serve the purpose equally well. Rhythm is just this process of subjectively accentuating and subordinating elements of a series. If the grouping factor just spoken of, does not exist objectively, then it exists merely subjectively, as an illusion. I purposely speak of *units numerically the same* without implying true periodicity of time because I mean to lay particular emphasis upon the principles of accentuation and subordination, and to regard time relations of any definite nature as not essential. Time, of course, is important in rhythmical grouping since these movements must occur in time, but this is no less true of any other form of activity.

I hesitate to speak of time as being the chief factor in rhythm primarily for the following reasons. If it is the chief factor, why is it that in ordinary life the group of five if made at all, is performed with greater difficulty than the four, or the six, or the eight, but is produced with greater ease than the seven which in turn offers greater difficulty than the six, the eight, the nine, or possibly even the ten? If time were the chief factor, there would be no reason for such a preference of groups. Moreover, if it were the chief factor, the accent would be of no value. There would be no reason for making

a distinction between physiological and psychological rhythms. And this failure to make such a distinction is the very mistake Bolton made.

Our movements in rhythm must correspond to the normal movements of our organs involved. The fact that our large members require more time than the smaller ones gives a basis for understanding the function of the accent, or why it is insisted upon in rhythm. There are other things which contribute a meaning to the accent, viz., the fact that in ordinary life we find it quite necessary to make lightly certain tentative or preparatory movements before the real purposive action is strongly executed. Further, there is the fact that we are bilaterally symmetrical. This affords us two means for executing like acts. One member of a pair usually becomes subordinated to the other. One makes the strong, but possibly unskilled, movements and leaves the production of the finer movements to the more skilful member. It is in this way that right or left handedness (and "footedness") exerts its influence. Our inherited structure is such that in performing purposive actions that require the use of the two hands serially we must often make weak and strong movements alternately, or one hand make two movements while the other hand makes only one.

In ordinary life two and three and their multiples are regarded as fundamental rhythms, and five, seven, eleven, etc., as no rhythms. They are regarded thus by MacDougall. He did not raise the question as to the possibility of *making* the groups of five, seven, or eleven seem rhythmical. Since they were not rhythms they were not instinctive and were therefore not considered in the investigation. The question of chief importance in my investigation naturally follows at this point. It is: Are rhythms instinctive? And if so, what kinds? In order to have a working basis for this investigation, I have formulated, provisionally, an hypothesis similar to the one which MacDougall was led to assume in view of the same facts. My assumption was that the simple movements of two and three and consequently their multiples are instinctive, and that the prime numbers higher than three are no rhythms at all.

One method of solving the problem is this:—I first tested the subject's ability to perform repeated movements in groups of two and three and then his ability to perform in groups of five, seven, eleven, or thirteen. Of course, the error in performing more complicated movements was greater. If, however, by performing certain purposive actions that will necessitate groupings of five, seven, or eleven elements, repeated

movements of these numbers can finally be performed with as much ease and accuracy as of two or of three, the conclusion must follow that there is no evidence whatsoever for saying that the group-movements of two and three are instinctive and that those of five and seven are not; but only that our life activities, especially early in life, call for certain actions more often than they do for other actions.

The first apparatus used for the experiment consisted essentially of two parts, that used for developing the new rhythms, and that used in testing the subjects for rhythm. The apparatus used for developing the rhythm is a simple upright frame measuring 20″ x 20″. This frame carries four button bells on the four pieces of its structure, one above, one below, one to the right, and one to the left. Any button can be reached conveniently by a person who stands before the frame holding his hand near the center. Almost any type of purposive action can be performed by striking the buttons with a heavy, short-handled rubber mallet. Generally the purpose is to sound one or more bells in a definitely prescribed order. No counting is allowed while doing so. Take, for example, the five-rhythm. The subject performs an activity on the frame which necessitates a combination of four light movements and one extra heavy one to sound a bell. We need only to imitate a very ordinary complex of movements which occur very frequently in our every-day activities. In order to make certain that we hit a small object,—such as a nail head, for example,—it is ordinarily necessary to make certain tentative movements before the final stroke is made. This very common form of activity is employed here to develop rhythm.

For the five-rhythm only the two horizontal buttons, that is, to the right and left, were used. Let us hit first the left-hand button. The tentative activity is a simple complex of two movements, the easy stroke in the direction of the button and the back movement. Then comes the heavy and then the rebounding or second back movement. This leaves the beating hand very near the right-hand button which is to be struck next. One movement to the left is therefore a necessary adjustment. This completes our group of four small movements and one heavy one, mentioned in the order of two light movements, one heavy, and two light movements. The same, but reversed, complex follows for the right button, again for the left, again for the right, and so on.

The second part of the apparatus, or that part which is employed in testing, consists of two electric buttons to be struck with the mallets, a kymograph to record the strokes and an

exposure apparatus showing thirty different stimuli, which are a mixture of letters and numericals. The exposure apparatus serves the purpose of distracting the subject so that he *can not count* his strokes. The subject must beat each time, and must call aloud the name of each stimulus when it appears. The test as to whether any movement is automatic is the subject's ability to produce it while giving attention to something else. Rhythm is automatic when it is not a voluntary counting of movements.

The purposive action on the frame to develop the five-rhythm necessitates a combination of four light movements and one extra heavy one to sound the bell as just described. This continues for ten minutes. The subject is then tested to see if the special activity has led to an improvement over a previous test. In the test he takes a rubber mallet in each hand, and with the right hand (if he is right-handed, otherwise he will use his left hand) he strikes the electric button every time a stimulus appears on the exposure apparatus. In order to make the movements rhythmical he accents every fifth element. This is done by bringing more muscles into play, by striking with both hands simultaneously. This test would be valueless, however, without the function which the exposure apparatus serves, as stated above.

The particular rhythm movements with which this division of the paper is concerned are three, five, and seven. I had subjects ranging in variety from a high-school student to a university graduate. Some were more mature than others. For this reason I expected a number of variations to appear. My intention was to obtain such a large number of subjects that I might disregard such variations, and that at the same time my conclusions might not be limited to one particular type of individual. The work was begun with twenty-one subjects. Only fifteen of these, however, completed the experiments.

The results are as follows. All were tested for various rhythms ranging from the two-rhythm to the nine-rhythm. The experimental result of all previous investigations of rhythm was again manifest beyond a doubt, viz., almost all subjects could perform the movements of two and three as well as their multiples and powers provided they were not too large numerically, but very few could produce the five and the seven rhythms. Some of the subjects, however, had the five and the seven rhythms from the very first. It is interesting and important to note here that the subjects highest up in the scale have the shortest reaction-time. It is important

because it shows that the more active subjects are best. It is, of course, probable that the more active have the five and seven rhythms developed to a greater degree than the less active individuals.

In the experiment, the three rhythm, which is numerically simple and which is ordinarily considered to be instinctive, is used as a control. The average error made by twelve persons on this rhythm is 13 per cent. That made on the five rhythm after the first ten-minutes exercise is 24 per cent. (The lowest person on the five rhythm made 60 per cent, while the highest made only 1 per cent of error.) After the second exercise, the error for the five rhythm was equal to that of the three rhythm. We must not take this equality too seriously because the individual fluctuations are considerable, and the number of test persons is small. An absolute interpretation of this fact of equality would mean that five is as much a rhythm as three. At the next test, however, the error on the five is 15 per cent (or 2 per cent greater than the three), at the next it is only 5 per cent, at the next 6 per cent, and at the next and final test only 4 per cent of error.

By the heretofore prevailing hypothesis three is an instinctive rhythm. The 13 per cent of error made in producing the three rhythm is to be contrasted with each of the above figures.

Per cent of error on five-rhythm 24 13 15 5 6 4
Per cent of error on three-rhythm 13

The figures for the seven-rhythm (average of twelve subjcts) are as follows:

Per cent of error on seven-rhythm 23.5 11 16 9 12 5
Per cent of error on three-rhythm 14

Since some of the second group of twelve subjects were new there is a slight difference in the error made on the three rhythm. In general, the figures on the five and the seven movements lead to the same conclusions, viz., *if three is innate, the five and seven are also innate; if the three is acquired, the five and seven are also acquired.* One important thing to notice is the fact that very little practice is necessary to enable a person to perform the five or seven-rhythm even better than the three which is by hypothesis instinctive. There seems to be no reason for concluding that rhythm of any sort is instinctive, but only that generally in life certain movements are called for more often than others and hence become more automatic, more habitual. Our bodily mechanism is so constructed that in performing purposive actions, in work and

play, the movements have usually a particular numerical make-up.

In contrast with MacDougall's notion that five and seven furnish no feeling of rhythm I must say that I found just the contrary to be true, or at least the contrary effect was produced. Although I do not deny that MacDougall's subjects failed to have that experience I am convinced that this feeling may be found even outside the psychological laboratory. The composer Tschaikowsky wrote musical measures of five-elements obviously because he enjoyed experiencing them. The strongest evidence is that after the experiment was over certain subjects invariably made greater errors when the exposure apparatus was going slowly than when it was going rapidly. If seven furnishes no feeling of rhythm, then the subject should be more able to produce the seven-groups when the machine goes slowly than when it goes rapidly. When it moves rapidly the subjects must react much more or altogether from their feeling of rhythm. This could not be done at first, because the subjects in question had found no opportunity to develop the feeling. The same is true for the five as for the seven. Introspections also supplemented this evidence. I believe that considerable confidence may be placed in the introspections for I was careful at all times to keep the subjects from understanding the problem. I told them at the end. This eliminated the effect of suggestibility on the part of the subjects. It was difficult if not impossible for them to tell when they were making themselves agreeable, and when disagreeable. In other words, they were not in any way influenced by what they thought I expected of them.

In order to show that the increase in accuracy upon the faster appearance of the stimuli is not due to a happening upon a *desired tempo* (the desired tempo has been of considerable interest to a number of investigators), I need only mention the fact that the fast tempo was considerably faster than any of the subjects chose to beat *in the absence of the exposure-apparatus*..

Time does not seem to play an essential rôle. As to the preferred tempo I found that there was a wide disagreement among the individuals, and that some individuals preferred different tempos from day to day.

It might seem that the chief thing to be learned from this experiment is that the major premise of many previous investigators may profitably be replaced by a new one. If we give up the old notion that rhythm is instinctive, then the new conception follows as the only and as the reasonable

alternative. It appears at this stage more evident than ever that the more important problems heretofore investigated may be looked upon as pseudo-problems. If habit is at the basis of rhythm perception, the time element in all rhythm should be expected to vary. General agreement as to tempo would be truly remarkable. The time required to execute the movements, the time between the movements, the tempo, etc., should make such fluctuations as can be accounted for only when the nature of the environment that called forth the rhythm is well known.

I do not mean to say that all the previous investigations are valueless. The old hypothesis, conducive as it was to the preponderance of pseudo-problems, nevertheless furnished a background for the prosecution of some very interesting investigations.

b. Second Experiment

Let us now continue our old problem of the origin of rhythm, and at the same time try to determine *the most efficient means of developing* rhythm. The methods here used are somewhat modified. Instead of having certain persons work on one problem and certain ones on another, I had all subjects work on both problems. One subject beat the five-rhythm on the electric buttons and counted while so doing; then he was tested to determine the improvement which had resulted from this practice of counting. To develop the seven-rhythm the same subject performed the special activity of seven on the frame and then was tested. One case alone can not furnish evidence as to the part played by purposive activities in rhythm formation, so another subject, instead of practicing the five-rhythm by counting his beats and the seven rhythm by special activity on the frame, reversed the process. This arrangement was extended to all the pairs of subjects. Particular pains were taken to make the work a purposive activity which required no counting. This was done in order that the ordinary conditions of life might be approximated. In life we do not ordinarily count our movements, for example, in eating, working, playing.

We may look upon the experiment already described as preliminary. It gives a more comprehensive view to the problems with which the following portion of the investigation is concerned. In the present case, instead of having such a large number of subjects as before, I dealt with a few intensively. By taking an interested few I secured an ideal degree of regularity and promptness. Heretofore my subjects had no incentive to

come to the laboratory except that I asked them. With some of them promptness was entirely out of the question. Further trouble which hampered the experiment was due to the apparatus.

Besides many minor changes, the exposure apparatus was set aside and a new one was made. In general, however, the whole testing apparatus is essentially the same as at the outset. Aside from a number of minor difficulties, there are one or two others which are due to the nature of the experiment. One of the most serious difficulties consists in the fact that any adult can and wants to strike the electric buttons faster than he can recognize and call the succeeding members of a series of promiscuously arranged letters. It requires some time to adjust one's self to call a new letter. This condition means that the apparatus must go more slowly than the hand ordinarily makes such movements. The calling makes the difficulty. The letters can be recognized soon enough; but unfortunately this fact can not help matters since recognizing alone affords no objective test as to whether the subject is conscientiously attending to the letters and not perhaps counting his strokes. One remedy for the difficulty was to use only three letters,—*S, O, R,*—which were chosen because of ease in pronunciation. These were so arranged that no two successive letters were the same, but they recurred without any regularity. The second remedy was to enlarge the slot so that two letters could be seen at the same time. This enabled the subject to call each letter just as it arrived at a certain point, designated by side arrows, and just as a hand movement is made, and to adjust his vocal organs for calling the next. I desired that the conditions be such that the subject could not devote his attention either to counting or to analyzing the period units he is asked to make, into smaller units. It may be true that when a subject acquires the seven-rhythm he may be able to describe his experience as two groups of two and then one of three, or any other convenient combination of small units such as three, three, and one. If this be the case, the test to determine whether he has acquired the seven-rhythm would be absolutely worthless provided he is allowed to make the same groups during the test. It might only show me that he has a *quasi* seven-rhythm made up of the groups two, two, and three, this being a three-rhythm with the accent produced by an extra stroke. In the first place I tried to avoid counting while the rhythm was being developed. Then for the test I arranged the difficulties of the apparatus so that there was no possibility of even the subconscious grouping just mentioned.

For the small children who acted as subjects, the arrangement was soon found to be adequate to do this; but for older people it was not difficult enough and another device was necessary. The three numerals 2, 3, and 4 were arranged at promiscuous places on the disc of the exposure apparatus so that no two of the same kind appeared twice in succession. The promiscuous arrangement was such that after the appearance of a stimulus some (irregular) time elapsed before the next one appeared. During this time the subject beat to the usual click of the metronome and continued to accompany each beat by calling out with the same loudness the stimulus that had last disappeared. By this means all forms of number imagery which might have any relation to the rhythm and practically all other imagery except the kinaesthetic images of the rhythm movement itself were eliminated.

Several factors had to be taken into consideration here. The one of greatest importance was the fact that the hand can execute the movement more quickly than the vocal chords can pronounce successive letters or numbers after time has been taken to recognize them. To overcome this difficulty the numbers were arranged in large intervals so that it was not necessary to recognize them each time the metronome clicked; and too, the range of expectation was narrowed down to one or another of three easily pronounced numbers. Another factor which we encountered consisted in a considerable tendency on the part of adults to count and thus to perform a mere quasi-rhythm. The introduction of the numbers tended more than the letters to offer a decided interference with the tendency to count. In fact, I feel quite sure that the tendency was overcome entirely. The letter system was just as satisfactory for the children as the number system was for the adults. Children do not possess the tendency to beat so fast as older people; and when they are quite familiar with the three letters involved they can pronounce them almost as well as the adult. The more coördinated a person's movements become, the faster he wants to beat.

Four small children, of five, six, six, and eight years of age respectively, were tested for the movements two to nine inclusive. As the successive groups became numerically larger, a correspondingly greater error was manifested. This increase in error was a gradual one. There was no sudden increase at five and seven as was observed with the adults. In fact, the error for each of the possible rhythms was very great, so that there seems to be little justification for saying they have any rhythm other than the two. The greater prob-

ability of the chance occurrence of the small group, two, in their life activities is possibly responsible for the small error made as contrasted with the larger groups. Two of the children did not continue the exercises long enough. The numerical results of the following tables refer, therefore, only to the other two.

The ability to perform the five-, six-, and seven-rhythms should be contrasted with that of three months previous.

	V-rhythm Errors	*VI-rhythm Errors*	*VII-rhythm Errors*
The five year old.....			
1st	88 per cent.	85 per cent.	90.5 per cent.
2nd	46 per cent.	64.5 per cent.	60.0 per cent.
The six year old.....			
1st	82.5 per cent.	84 per cent.	88.5 per cent.
2nd	69 per cent.	80 per cent.	40 per cent.

There is no marked deviation from the results of the first test; but for the second test after three months of practice one significant fact is manifest. Let us consider the two individual results thus:

	V-rhythm Errors	*VI-rhythm Errors*	*VII-rhythm Errors*
H. Five year old (error in second test)....	46 per cent.	(64.5 per cent.)	60 per cent.
D. Six year old (error in second test)....	69 per cent.	(80.0 per cent.)	40 per cent.

(1) The error made on the six-rhythm is in each case greater than either the five or the seven-rhythm. *The only possible exercise here to develop this rhythm during the investigation is the mere beating on the buttons for the test.*

(2) On the five-rhythm H. made a smaller error by 23 per cent. *H. used the purposive method to develop the five-rhythm while D. used the counting method.*

(3) On the seven-rhythm H. made a greater error than D., by 20 per cent. *Here H. used the counting method and D. used the purposive method.*

The exercises were taken twice each day, as nearly as this could be done during the three months. The periods of exercise for each subject were five minutes in each of the three methods. Pains was taken to make the conditions of work for the two subjects identical. They worked during the same times of day, for the same periods, and were tested during the same hour. A little more might be said about the three methods just spoken of. 1. The test method consisted in the rhythmical striking of two buttons on the table, the unaccented

strokes made on one button with one hand only, the accented strokes on both buttons simultaneously with both hands, with counting excluded by the requirement of recognizing and calling out the stimuli exposed at every beat, as described. *2.* The counting method consisted in rhythmical beating just as in the test method; but counting was required, and the exposure apparatus was, of course, out of function. *3.* The purposive method consisted in performing one of the specially chosen actions on the frame. For the five-rhythm, this activity has been described. For the seven-rhythm, it was the one corresponding to the following task: Strike the button to the left lightly (first movement); move to the right to suit the rebound (second movement); make a preparatory movement of adjustment to the left (third movement); strike the button to the right lightly (fourth movement); rebound as before (fifth movement); make a preparatory movement to the right (sixth movement); and now make a heavy stroke in space stopping suddenly near the center of the frame, to be followed by all the inner muscular adjustments (seventh movement, period completed) necessary to perform the same action in the vertical instead of the horizontal plane. Perform the same action again in the horizontal plane, again in the vertical, and so forth. Such a purposive action is comparable to that of a workman serving a particular machine in a factory, where, of course, he would not count, since the task can be done perfectly without thinking of any numbers. No numbers are found in the above description of the task. Similarly, what the subject in the laboratory has in mind does not include a series of numbers, but is simply the idea of the purpose, of the prescribed task.

It now remains for me to tell a part of the story which the tables do not show. Let us take a particular case. H. had acquired the five-rhythm so well that in the test he was making no errors when suddenly he stopped to laugh about and tell me how his dog 'Jack' went down stairs with all of his legs bent back. For a short time after this pause no particular movement was executed until he happened on the seven (which was by this time a rhythm) when he beat five groups of seven elements each. Ordinarily when H. happened on the seven, he usually made quite a series of seven-groups; but if he should be beating the seven-rhythm when the confusion occurred he would very likely start the five, and in that case he would beat a much longer series of five-groups than the seven-groups in the reverse transition. The same general statement can be made of D. except that she (since the seven

had become easier for her) would beat the seven for a longer time than the five. Toward the latter part of the three months, I could always start the children beating the rhythms five or seven by working their arms. I could not induce them to beat three-, four-, eight-, or nine-rhythms by the same process, or any other, for that matter. As soon as I released their arms they would very soon begin beating either the five or the seven.

After a lapse of five months, H. and D. were again tested for the rhythms two to nine inclusive. Those rhythms which were learned by the purposive method were reproduced even better than at the previous test. At this test there seemed to be nothing new concerning the other possible rhythms, except that those which had been learned by the counting method, and the six-rhythm, were performed but slightly better than before any practice had been acquired. What little had been learned about them seems to have been forgotten almost entirely.

The subjects were again allowed to practice. This time only the purposive method was used since the relative inefficiency of the counting method was quite apparent; and as usual H. developed the five-rhythm and D. the seven-rhythm. At the close of seven exercises of about twenty minutes each they were able to produce the rhythms by making only an occasional error.

c. Third Experiment

Similar experiments with adults and with more complicated apparatus for adults furnished even more striking confirmations of the conclusions to be drawn from these experiments with children. More difficult rhythms were here taken into consideration. One person practiced the thirteen-rhythm on the frame, and used the counting method for developing the eleven-rhythm, while a second person reversed the process, that is, beat the eleven-rhythm on the frame and counted for the thirteen-rhythm. After seventeen periods of practice *each of these subjects made very little error in producing those rhythms which were developed by the purposive method, that of beating on the frame. On the other hand, no evidence could be found that rhythms were developed by the counting method.* It was found that instead of beating the rhythm which he had tried to develop by the counting method, the subject beat either no particular rhythm or the one learned by the purposive method. This outcome seems truly remarkable, especially since in the experiment with the small children there appeared

to be evidence to justify the use of the counting method. The adults *not only succeeded in producing those rhythms* which they had learned by the purposive method, but they were *conscious of almost every mistake* made during the tests. These mistakes, however, were so few that in one case the error was 5 per cent, while in the other it was 7 per cent, the greater error being made by the person who beat the simpler rhythm, viz., the eleven-rhythm.

C. Practical Applications

In view of these results it may be interesting to discuss the " Rhythmical Gymnastics " of Jacques Dalcroze. Dalcroze is a teacher, formerly at Geneva but now at Dresden. His plan is to train his pupils to produce rhythms well, before they begin the study of music. The pupil's training is identical to what has been called *the counting method.* Dalcroze and his followers are convinced that great benefit is being wrought by this method, but to substantiate this conviction they have resorted to mere opinion. No test has been made to determine whether or not the rhythms were actually acquired by the Dalcroze method. It seems reasonable to suppose that the Dalcroze method is not altogether fruitless. The last described experiments on adults which seem wholly to discredit this means, contained only the small number of seventeen exercises. If this number were considerably increased it is possible that something would finally be gained even by the counting method. At least since this was the case with the small children there is no reason to believe that the exercises given by Dalcroze to his pupils are entirely fruitless. The writer is forced to the conviction, however, that the purposive method or method of ordinary life is the more efficient one.

The purposive method eliminates all forms of imagery except the kinaesthetic images of the movements. In the counting method, the analysis is not so complete; for by it are developed two forms of imagery, viz., kinaesthetic and number imagery. There are many activities which demand kinaesthetic imagery, but not number imagery. (My test may be cited as an example of such an activity.) For a person who possesses both kinds of imagery, the test is markedly a new activity, whose performance necessitates the elimination of the number images. It is an easy matter to inhibit these, but such an inhibition seriously disturbs the rhythm. To a certain degree the rhythm must be relearned. On the other hand, the purposive method fits the person for any activity which involves the rhythm previously learned.

Learning to perform, by means of one or many different members of the body, a purposive action of *n* movements of which one is "bigger" than the (*n*-1) others and being then able to tap on the table in such a manner as to accentuate every *n*th tap, involves obviously a *transference* of an identical form of activity from one part of the organism to another. The extent to which and the ease with which a well-learned rhythm may be carried over from the hands to the speech organs, for example, and back again to the hands with the result that one has difficulty in recognizing it, is worthy of note. The writer was able to perform the six-rhythm without error, in the ordinary way of beating five times with one hand and then once with both. After half a dozen ten-minute exercises, the same rhythm could be beaten accurately in a new and more complicated way which will at once be described. The training which is necessary to bring about the transition is the following. Take this "sentence" of six simple words. *I I can not go go.* These six words take the place of the above six beats. While these words are spoken, the right hand beats three times while the left hand beats only twice. The right hand beats to the first *I,* the *can,* and the first *go,* while the left hand beats to the first *I,* and the *not.* In musical terminology this is a "triplet," that is, three notes played with one hand in the time of two notes played with the other hand. This activity is deceiving, for it seems as though the human organism is producing a three-rhythm with one member while with another it is simultaneously producing a two-rhythm. Careful consideration of the above method of acquiring this ability, however, reveals the fact that this is not the case, but that this is only an example of the six-rhythm with embellishments added. Instead of first one hand and then both as in other cases, each hand has here a different share of the six movements to execute. Where the hands fail to execute one of the elements of the six-rhythm, the muscles of speech execute it, and complete the six elements of the period.

The 'quintuplet' of the form five to three was learned by people in a similar manner. We employed the following sentence, which contains fifteen elements. *And now you may see*
R *L*
I've crossed the big sea and got to New York. The letters
R *R* *R* *R*
L *L*
R and *L* below the sentence indicate the words accented by the right hand and those accented by the left hand. In either

case, these movements of the hand are real accents (secondary, of course, to the one main accent of the whole sentence), since each brings into play more muscles than the muscles of the speech organs which function with every syllable. Only the very superficial observer would describe this process simply as the production of the five-rhythm with the right hand and the simultaneous execution of the three-rhythm with the left hand; for this process is really the fifteen-rhythm.

The method of the previous experiment was originated by Mrs. Fannie Church Parsons of Chicago and applied to kindergarten children. There seems to be, however, no reference to her work in the literature. This method does not develop needless number images, but it does develop, in addition to the kinaesthetic imagery, visual and auditory images of the words of the sentence. These latter images may be useless, and perhaps they are a hindrance in such an activity as my test involves. Fortunately these images do not stand much in the way since they can easily be inhibited without seriously disturbing the rhythms. This system, therefore, has decided advantages over the counting-method and may be comparable in efficiency to the purposive method as previously described.

The Dalcroze method of teaching rhythms has been referred to in this paper as a *counting-method*. Some explanation is here necessary since Dalcroze says: "Ich verlange garnicht, dass man zählt. Es sind die Unbegabten welche zählen. . . . Meine Methode ist Sache der muskulären Erfahrung." ("I do not require any counting of my pupils. It is the untalented who count. . . . My method is a matter of muscular experience.") The hopes of Dalcroze are here plainly stated; but whether he fully realizes them in practice is open to question. In one sense his statement as to the untalented is a confession that counting is by no means discouraged by him or made unnecessary, for his pupils can and do count. I have spoken with a number of people who have acquired rhythms by his method; and the following stubborn fact is always to be observed. In order to reproduce the rhythmical actions learned under him, which during the course of time have almost been forgotten, they invariably count. Number images are the means for reproducing the movements. This shows that in his teaching Dalcroze has not succeeded in making the 'muscular experience' independent of number images. I have no reason for supposing that these people with whom I have spoken are untalented. I rather think the method is such that all have an inducement to count. Perhaps still another quotation is here appropriate. "Wenn man den Rhythmus seiner

Glieder empfindet, braucht man nicht mehr zu zählen." ("As soon as one perceives the rhythm of his bodily members, one does not have to count.") So it is evident that the students start out by counting; and no one but the pupils themselves can say whether or not they continue to count throughout their training. It is no wonder, then, that counting is the means for reproducing the old movements. Under these circumstances I doubt if Dalcroze's method of teaching 'rhythmical gymnastics' deserves to be regarded as a specifically new method. His success as a teacher seems to be due to personal qualities rather than to excellence of method.

Dalcroze says plainly, "Ja sicher, Rhythmus ist erblich." ("Yes, surely rhythm is hereditary.") If the rhythms are inherited, then one method is as good as another to describe what action is at any particular time desired. Why, then, not use counting since it is the simplest means? But according to experimental results, included in this paper, the avoiding of number images seems to be a matter of expediency. Dalcroze is aware of the possibility of eliminating counting after the rhythms have been, so to speak, found by the students. He conforms to natural conditions in so far as he takes pains to allow (large) movements of small frequency to be made by large body members and (small) movements of greater frequency to be executed by the smaller members. Thus it is possible for him, after persistent and consistent work, to emancipate himself from all counting. This conforming to anatomical conditions, as just stated, seems to be the whole of the 'Dalcroze method.'

D. Summary

All experimental evidence seems to point to the conclusion that rhythm is acquired by each individual, and that it is not inherited. Biological conditions,—for example, the anatomical fact that we are two-footed, two-handed, and generally two-sided, not three-cornered or star-fish like beings,— are favorable for the development of those rhythms which have usually been considered to be instinctive, while the other rhythms can be acquired only under special, somewhat artificial conditions. The best means for developing rhythm is that which approaches our ordinary life activities. In the development of a rhythm, the motor activity of the skeletal muscles plays the most important rôle. For this reason, the larger movements of a purposive activity are much more conducive to the production of rhythm than the smaller movements which accompany the almost purposeless activity of the counting method.

I feel greatly indebted to the many people who persistently acted as subjects in this experiment; and I am especially grateful to Professor Max Meyer who has been ready at all times to advise and assist me, and who even devoted much of his time to serving as subject himself.

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